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Preventing Drug Use

Beau Kilmer and Rosalie Liccardo Pacula

7.1 Introduction

There is a wealth of evidence suggesting that substance use and poverty are closely connected. Surveys of the homeless show staggering rates of alcohol and drug dependence among this marginalized population (Greene, Ennett, and Ringwalt 1997; Wenzel et al. 2004). Similarly, studies of the household population find that female welfare recipients are twice as likely to report illicit drug use when compared to women with dependent children who did not receive assistance (Jayakody, Danziger, and Pollack 2000; Pollack et al. 2002). Even data from the 2007 National Survey of Drug Use and Health (NSDUH), which generally reflects the household population (although efforts are taken to include individuals living in homeless shelters and other group homes), supports the positive association between illicit drug use and poverty. As shown in figure 7.1, individuals living below the federal poverty line are 50 percent to 100 percent more likely to report use of an illicit drug in the past month or dependence/abuse in the past year than individuals with incomes exceeding 200 percent of the federal poverty threshold. Alcohol dependence or abuse also appears to be slightly higher among those with incomes below the federal poverty threshold, but the results are not statistically significant. Only current use of alcohol in the previous month, which combines casual drinkers with heavy users, shows a negative association between use and poverty.

What is not clear from these data is whether substance use and abuse actually cause poverty. It may be that those experiencing severe poverty

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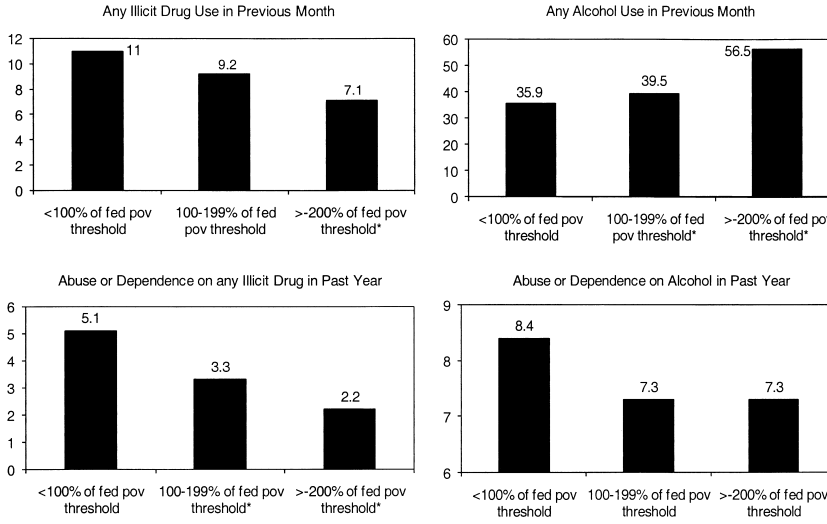

Fig. 7.1 Substance use and dependence by income level, 2007 National Survey on Drug Use or Health (NSDUH)

Notes: Based on online analysis of the 2007 National Survey on Drug Use and Health (N = 55,435). Asterisks (*) after a bar label indicate statistical significance at the 95 percent level. The sample is selected from the civilian, noninstitutionalized population of the United States aged twelve and older, including residents of noninstitutional group quarters such as college dormitories, group homes, shelters, rooming houses, and civilians dwelling on military installations. The federal poverty measure used here was reported in the public use data. According to SAMHSA's codebook, the measure is constructed using information about the family size, number of children, and total family income. The federal threshold is based on data in 2006, as reported by the U.S. Census. Abuse and dependence are based on DSM-IV criteria.

use alcohol and illicit drugs to cope with the stress of being poor. Also, some argue that public programs may foster economic dependency and even encourage substance use by providing resources to support a drug habit and reducing incentives to work (Shaner et al. 1995; Phillips, Christenfeld, and Ryan 1999); however, recent evidence does not fully support this notion (Rosen et al. 2006; Chatterji and Meara 2007).

The limited scientific literature examining a causal connection between poverty and substance use remains unsettled. Most studies examining the topic examine the contemporaneous relationship between substance use and labor market participation and/or current earnings, and the results are generally mixed. Part of the problem is that substance use can influence both earnings and labor market participation indirectly through health, education, marriage and fertility, and job experience (e.g., Kandel, Chen, and Gill 1995; Kaestner 1999; Fergusson, Horwood, and Swain-Campbell 2002; Ringel, Ellickson, and Collins 2006). So it can be difficult to ascertain the full effect of substance use on poverty status in a manner in which

causality can be clearly attributed to the use of substances. Importantly, one study finds that reducing drug use among Aid to Families with Dependent Children (AFDC) participants to the level of nonparticipants would actually reduce welfare participation by 3 to 5 percent (Kaestner 1998). This study suggests that current substance use does influence welfare participation.

The question remains, however, whether adolescent substance use contributes to adult poverty. The direct connection is difficult to make given the large number of potentially confounding factors, such as criminal involvement, early pregnancy, and not finishing high school. While a few studies have shown through analyses of selective cohorts that adolescent use of hard drugs during high school is correlated with lower job stability and/or higher unemployment later in life (Kandel et al. 1986; Newcomb and Bentler 1988; Schulenberg et al. 1996; Ellickson, Tucker, and Klein 2003), the samples are typically small, lack a quasi-experimental design, suffer from attrition bias, and only use vague outcomes correlated with poverty rather than poverty status itself.

This chapter considers whether substance use prevention programs targeted at adolescents can influence the probability of experiencing poverty as an adult. Because we are not aware of any studies that have directly addressed this question, we draw conclusions from two different literatures: (a) The literature on the effectiveness of programs intended to prevent substance use among adolescents, and (b) the literature on the effect of substance use on educational attainment and labor market outcomes. The next section begins with a discussion of the etiology of substance use, which helps the reader understand why the timing of substance use initiation and escalation complicates studies attempting to assess the causal effect of this use on later life outcomes. Section 7.3 presents an overview of interventions intended to prevent adolescent substance use, and section 7.4 presents our specific exclusion criteria for the program review we conduct. Section 7.5 reviews the experimental studies of these prevention programs, with a special emphasis on the long-term outcomes. Section 7.6 reviews the literature on how substance use influences labor market outcomes as well as how substance use influences educational attainment. Section 7.7 summarizes these findings and lists some ideas for future research in this field.

7.2 Background on the Etiology of Substance Use

According to information from NSDUH, 29 percent of sixteen- to seventeen-year-old adolescents report use of alcohol in the past thirty days, and approximately one in five (19.4 percent) report binge drinking in the past thirty days (Substance Abuse and Mental Health Services Administration

[SAMHSA] 2008).[1] Rates of illicit drug use are similar to binge drinking rates as 16 percent of youth ages sixteen to seventeen report use of an illicit substance (mostly marijuana) in the past month. Given the illegality of alcohol and drugs for this particular age group, the relatively high use rates are often viewed as troubling. When considered within the context of other decisions made by youths at this age regarding finishing high school, applying to college, and engaging in unprotected sex, the relatively high prevalence rates become even more disconcerting.

Figures 7.2 and 7.3 illustrate the trends in consumption among tenth grade students from the Monitoring the Future Survey since 1991.[2] Figure 7.2 shows that the current use rate for any illicit drug (primarily marijuana) in 2007 is below its peak but still nearly 50 percent higher than it was in 1992. Figure 7.3 shows that daily use of marijuana in 2007 (2.8 percent) is also below its peak value (4.5 percent), but still more than three times as high as its low value in 1991 (0.075 percent). There have also been fluctuations in the prevalence rates for being drunk over this period, but they have not been as dramatic as the fluctuations in marijuana use.

Rates of initiation for the various substances confirm the notion that substance abuse is often a problem that begins at a very early age. Whether discussing cigarettes, alcohol, or illicit substances, substance use typically begins during adolescence for many, peaks during early adulthood (ages eighteen to twenty-five), and then (in the case of illegal substances) diminishes in the late twenties and early thirties (Kandel and Logan 1984; Johnston et al. 2005; SAMHSA 2008). These patterns of use across substances in the general population are amazingly consistent across time, locations, gender, and race/ethnicity, although the age of initiation can differ in important ways across the substances. For example, cigarettes, alcohol, and inhalants are generally substances that are initiated prior to illicit substances and can begin as early as fifth and sixth grade (Chen and Kandel 1995; Johnson and Gerstein 1998). As for "harder" substances, the average age of first use among the household population for marijuana was 17.6 years, for cocaine and ecstasy 20.2 years, for heroin and pain relievers 21.2 years, and for tranquilizers 24.5 years (SAMHSA 2008).

Age of initiation is a particularly important indicator of problematic substance use. Numerous studies have shown that early initiates are at greater risk of serious mental illness, poor schooling outcomes, and dependence (Bray et al. 2000; Patton et al. 2002; Wells, Horwood, and Fergusson 2004;

1. Binge drinking refers to the consumption of five or more drinks in a single drinking occasion (i.e., within a few hours). Rates are even higher among eighteen- to twenty-year-olds, where 50.7 percent report drinking in the past thirty days and 35.7 percent report binge drinking in the past month.

2. The Monitoring the Future survey is a school-based survey of students while the NSDUH is a survey of the household population. By focusing on use rates among tenth graders, we hope to capture those who are still required to stay in school due to their age.

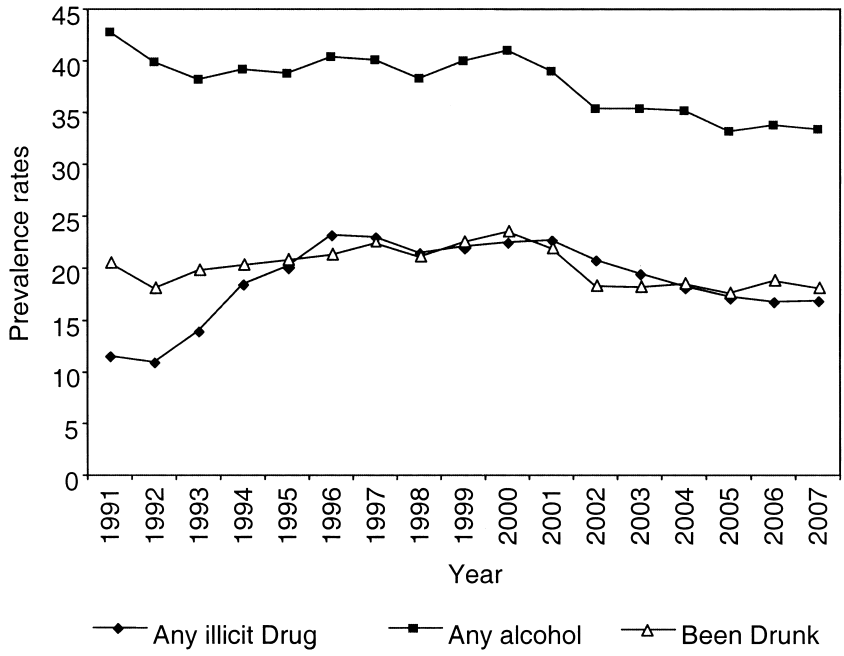

Fig. 7.2 Thirty-day prevalence of substance use among 10th graders
Source: Johnston et al. (2008).

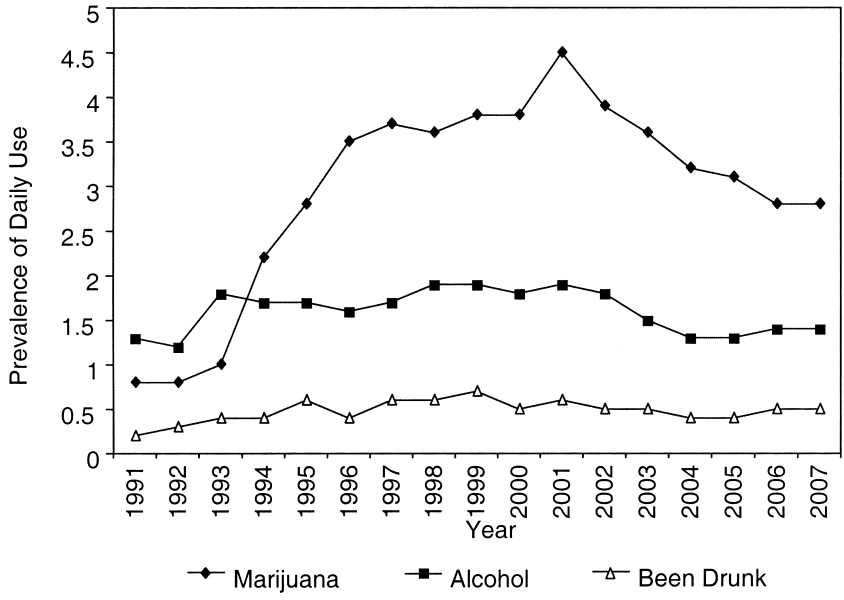

Fig. 7.3 Rates in daily use of substances among 10th graders
Source: Johnston et al. (2008).

Patton et al. 2007). Importantly, these are all outcomes that are also highly associated with poor labor market outcomes and reduced income. In the case of mental health problems and poor schooling outcomes, the evidence regarding the direction of the causal association remains mixed, as many studies show that heavy substance use precedes these outcomes (Hawkins, Catalano, and Miller 1992; Fergusson and Horwood 1997; Coffey et al. 2003). In the case of dependence (which is a diagnosable mental disorder) the causal association is actually clear and descriptive evidence from the NSDUH survey confirms the result. In 2007, 15.9 percent of adults who reported that they initiated alcohol use at fourteen years or younger met DSM-IV criteria for alcohol abuse or dependence, compared to only 3.9 percent of adults who first had alcohol at age eighteen or older. Similarly, 12.9 percent of adults who reported first trying marijuana prior to the age of fifteen met DSM-IV abuse or dependence for an illicit substance, whereas only 2.7 percent of adults who initiated marijuana after age eighteen met the criteria. The link between early initiation and subsequent dependence as well as duration of dependence has held up in multivariate analyses of data from the United States and other countries (Fergusson, Horwood, and Beautrais 2003; Pudney 2004; van Ours 2006; Patton et al. 2007; van Ours and Williams 2007). Thus, programs that can delay initiation past certain critical ages will reduce subsequent dependence on these substances, which may improve future labor market outcomes and reduce poverty.

7.3 Taxonomy of Interventions to Prevent Adolescent Substance Use

Prevention programs are typically divided into three categories: Universal (for the general population), selective (for those at risk or just beginning to use), and indicated (for those already using). When discussed among policymakers, they can also be thought of in terms of the context in which they are provided (school-based prevention, family-based prevention, and community prevention). We focus on this alternative categorization here.

7.3.1 School-Based Programs

The vast majority of middle school students receive some sort of universal school-based prevention designed to reduce short-run and long-run demand for alcohol, tobacco, and illegal drugs (Gottfredson and Wilson 2003). Universal and selective school-based programs have also been developed for elementary and high school students. The more successful programs typically include booster sessions for kids as they progress in school. Decisions about the type of program to adopt are usually made at the school-district level, where officials are often courted by vendors for particular programs. The curriculum of these programs can be classified into three general types: information only, skill building (understanding social influences and learning how to respond to different situations, including resistance training),

and normative education (changing perceptions about substance use norms) (Manski, Pepper, and Petrie 2001). Programs are taught by teachers, peers, outside speakers (e.g., police officers, trained health educators), or some combination of these depending on the program. Sessions can occur within classrooms or in auditoriums with the entire student body.

7.3.2 Family-Based Programs

These programs generally focus on families with a child or parent who is currently using drugs or is at high risk of doing so. Interventions can range from psychotherapy to programs intended to improve intra-family communication and promote a discussion about the consequences of consumption. Sessions can occur in a variety of locations, including a physician's/therapist's office or inside the home. It is also important to note that many school-based prevention programs include components intended to extend the discussion about substance use to the home.

7.3.3 Targeted Community-Based Programs

Targeted community-based programs are those that target a specific population within a community that may be at particularly high risk for drug use and abuse. Mentoring programs, like the YMCA and Big Brothers Big Sisters, which are intended to promote healthy relationships by offering positive role models to disadvantaged youth, are one such example.[3] Other types of programs that also fall into this category include specific law enforcement activities (such as neighborhood policing), drug treatment, and criminal justice interventions. While not frequently viewed as prevention, these programs do in fact aim at preventing access to drugs (e.g., preventing street markets in certain neighborhoods in the case of law enforcement) or relapse of drug use among youths who have already initiated (in the case of treatment and criminal justice interventions). A wide range of tools have been used through these alternative systems. For example, criminal justice interventions targeting high-risk youth vary from drug education, to treatment diversion programs, to boot camps.

7.3.4 Universal Community-Based Programs

This category includes programs and policies that provide universal coverage to all individuals living within a community, regardless of their risk of use. Examples of universal programs include mass media campaigns, restrictions on sales to minors, policies raising the price of legal and illicit substances (including general drug enforcement), and advertising restrictions. All of these approaches represent community-wide attempts to prevent the initiation of or reduce use of alcohol, tobacco, and/or illicit drugs.

3. The popularity of these programs has grown six-fold in the past ten years and it is now estimated that over 3 million adolescents currently have adult mentors (Rhodes 2008).

Also included in this category of programs are multipronged community-level initiatives intended to provide comprehensive strategies for reducing substance use. While this chapter focuses on specific interventions rather than attempts to combine many interventions, it is important to recognize that the reported effectiveness of an initiative may differ if it is part of a comprehensive strategy.

Two additional types of programs that are frequently referred to in prevention circles have not yet been mentioned: drug and alcohol testing, and brief interventions. These programs can be implemented in a variety of different settings, including schools, health care facilities, places of employment, and community criminal justice settings, and hence do not fall neatly into the aforementioned classification. Drug and alcohol testing involves testing urine, sweat, breath, blood, or hair to identify the use of these substances. It is important to note that simply detecting whether someone is using alcohol or drugs does not influence consumption itself; a change in behavior depends on how that information is used. If an individual believes there will be sanctions associated with testing positive (e.g., exclusion from after-school sports), this could influence use if the expected sanction is larger than the expected benefit of consumption (Becker 1968). If detection forces someone into treatment or into a fruitful discussion with a caring adult, this may also influence future consumption.

Often rooted in motivational interviewing, the goals of brief interventions (BIs) are to help the users identify problem use and enhance their motivation to change this behavior (Tevyaw and Monti 2004). These short interventions (often less than thirty minutes) range considerably in terms of their content, target population, delivery mechanism, setting, and goals (e.g., reduced risk-taking behavior, engagement in treatment); thus, they represent a heterogeneous group of programs. While BI is often associated with primary care settings and emergency rooms, the model has recently been adapted to several different settings for adolescents, including schools, shelters, and teen courts (Baer, Peterson, and Wells 2004; D'Amico and Edelen-Orlando 2007; D'Amico and Stern 2008).

7.4 Inclusion Criteria Used for Review of Prevention Program Effects

Since many policies and programs can be construed as having preventive effects, the list of potential programs to evaluate is quite extensive. Thankfully, many reviews and meta-analyses of a variety of prevention programs already exist (e.g., Manski, Pepper, and Petrie 2001; Caulkins et al. 2002; Gottfredson and Wilson 2003; Faggiano et al. 2005; Gates et al. 2006). Most conclude that skills-based prevention programs (in schools or elsewhere) are effective at deterring early-stage drug use, by delaying initiation and reducing the frequency of tobacco, alcohol, and marijuana use among young adolescents during the period in which the youth are engaged in the programs,

but the effect sizes are small (Manski, Pepper, and Petrie 2001; Faggiano et al. 2005). More importantly, few studies provide evidence of sustained effects after the programs end (Manski, Pepper, and Petrie 2001; Caulkins et al. 2002; Faggiano et al. 2005). Even fewer are independently evaluated in terms of their long-term effects. Indeed, a recent National Research Council (NRC) panel was quite pessimistic when summarizing their review of the prevention literature, stating:

> At least 20 reviews and meta-analyses of drug prevention programs were published during the 1980s and 1990s. The most recent of these generally conclude that substance abuse prevention efforts are 'effective' for preventing substance use, in the sense that the studies reviewed report statistically significant differences between subjects receiving and not receiving the preventive intervention on some measure of substance use, at least immediately following the termination of the prevention activity, and in rare cases months or years beyond that point. However, certain practices in the reporting of original research and in the summaries of these findings have tended to overstate the effectiveness of prevention activities. (Manski, Pepper, and Petrie 2001, 213)

Because we are interested in considering whether drug prevention programs can be used as a means of reducing adult poverty, we are particularly interested in understanding whether drug prevention can result in either (a) sustained reductions in substance use, or (b) delayed initiation past the end of the program. Either of these outcomes might then translate into positive schooling and labor market outcomes, which should reduce the likelihood of experiencing poverty as an adult. We therefore limit our review of adolescent prevention programs to those that meet the following three criteria: (a) they have been evaluated using a randomized-controlled trial, thereby increasing the reliability of findings even if conducted by the developer of the program; (b) they include follow-up information related to substance use at least twelve months after the end of the program; and (c) they were conducted within the United States. To identify programs included here, we conducted our own literature review and drew on reviews by Skara and Sussman (2003); Foxcroft et al. (2003); Faggiano et al. (2005); Gates et al. (2006); and D'Amico and Stern (2008).

Application of these inclusion criteria means that some prevention programs are not considered. In particular, law enforcement strategies are excluded as most are not rigorously analyzed with a focus on longer-term outcomes. We similarly do not include brief interventions as we are unaware of studies examining the long-term outcomes from these programs for adolescents. Interventions that were not generally considered drug prevention (e.g., Head Start) are also not included. The notable exception is Big Brothers Big Sisters, which has been evaluated using a large sample, randomized design, long-term follow-up, and has demonstrated sustained effects on

substance use over time.[4] We do not consider multicomponent community-level prevention since is it is extremely difficult to identify the main mechanism driving the change (the message itself, parental involvement, accountability, etc.). Finally, as the focus of this chapter is on more traditional prevention programs, we do not review the extensive literature on treatment programs. Readers interested in reviews of the treatment literature should consult Manski, Pepper, and Petrie (2001, chapter 8); Aos et al. (2006); and the National Institute on Drug Abuse (2009).

Most program evaluations considered in this chapter do not include outcome information related to education and adult employment because (a) they are usually not the primary outcomes of interest, and (b) they tend to focus on short-term outcomes. When available, we do report this information. We also include a summary of the long-term effects of one nonexperimental program, the Seattle Social Development Program, since it includes rich information on substance use as well as on work and school outcomes.

7.5 Review of the Long-Term Effects of Prevention

7.5.1 School-Based Programs

Table 7.1 presents the results from long-term evaluations of randomized controlled prevention experiments with schools or students. The first column includes the name of the program and the second column includes information about grades covered, number of sessions, and theoretical approach of the program.[5] All programs included in the table have a follow-up evaluation that takes place at least one year after the prevention program ended. For programs that included multiple follow-ups (i.e., "waves"), we only present the information from the last wave available since we are primarily interested in whether program effects can be sustained over time.[6]

There is no evidence suggesting that school-based prevention programs have any long-term effect (> five years) on marijuana use. While some of these programs do appear to have an impact several months after the intervention (e.g., Adolescent Alcohol Prevention Trial [AAPT], ALERT), the six-year follow-up for ALERT and Life Skills as well as the four/five-year follow-up for Project Towards No Drug Use (TND) showed no effect on marijuana use. An independent evaluation of the Project ALERT curriculum delivered to students in eight Pennsylvania middle schools by outside

4. The Head Start program is evaluated for its long-term effects on poverty in another chapter in this monograph.

5. We heavily rely on Skara and Sussman (2003) for their descriptions of the program approach (e.g., comprehensive social influences, resistance education) and refer readers to their review for more specific details on many of the programs listed in table 7.1.

6. The one exception is AAPT, where the five-year evaluation did not include information about marijuana use; thus we include information from the one-year follow-up (Hansen and Graham 1991).

program leaders demonstrated no effects of the curriculum on substance use at the twelve-month follow-up (St. Pierre et al. 2005). However, the authors admit that lack of an effect may be attributable to differences in how the program was administered (particularly the use of outside program leaders rather than teachers familiar with the students). The study raises important questions regarding the reliability of prevention programs when diffused broadly and the importance of independent evaluations of the programs. Because no other prevention programs were similarly evaluated under alternative conditions by independent evaluators, we do not dismiss the results of Project ALERT vis-à-vis the other program effects. Finally, while Project Towards No Drug Use did find a small effect of the program on hard drug use at the four- or five-year evaluation, the authors note that this effect should be interpreted cautiously because they did not find a sustained program effect in years two and three, suggesting that their result in waves 4 and 5 might only apply to the selective sample that remained in the study over the full period (i.e., "attrition bias").

The effect of these programs on long-term alcohol use is slightly better. The evaluation of Life Skills found no effect on frequency of use, but that it did reduce the probability of being drunk in the previous thirty days at the six-year follow-up. An evaluation of the Adolescent Alcohol Prevention Trial (AAPT) using five waves of data and latent growth curve modeling produced more promising results (Taylor et al. 2000). The analysis found that the seventh grade program had a beneficial effect on lifetime alcohol use, recent alcohol use, and lifetime drunkenness through the eleventh grade. The authors also found that those randomly assigned to the normative education program had lower rates of growth for self-reported alcohol use. The long-term evaluations of ALERT and TND found no effects on alcohol.

The results with respect to smoking appear to be very program-specific. The Hutchinson Smoking Prevention Program is based on the social influence model and includes sixty-five sessions over the course of fourth to tenth grades (Peterson et al. 2000). The authors were able to follow-up with 94 percent of the original sample at two years after high school and found that the program had no effect on daily smoking or other smoking outcomes. Projects ALERT and TND also did not find any long-term effects of prevention on smoking behaviors; however, both of these programs had lower retention rates (57 percent and 46 percent, respectively) that may have influenced these results.

The six-year wave of Know Your Body (two hours per week of teacher instruction for every school week from fourth through ninth grades; 384 total sessions) found that 13.1 percent of the control group initiated use in ninth grade, compared to 3.1 percent of those assigned to the intervention (Walter, Vaughan, and Wynder 1989). However, it should be noted that they were only followed up with 65 percent of those surveyed at baseline and the evaluation was conducted at the end of the program (ninth grade). Thus,

Table 7.1 **Long-term results of experimental evaluations of school-based prevention programs in the United States**

Study	Program description	Evaluation design and sample
Adolescent Alcohol Prevention Trial (Taylor et al. 2000; Hansen and Graham 1991)	Ten sessions in 7th grade; comprehensive social influences.	Students in twelve schools in Los Angeles were randomly assigned to one of four prevention conditions (by school). Five waves of longitudinal data were obtained for 33.5% of the 3,027 students (through 11th grade). Authors use structural equation modeling to address data missing from waves 2–5.
D.A.R.E. (Perry et al. 2003)	Ten sessions in 7th grade; resistance education is taught by a police officer in the classroom.	Twenty-four middle schools in Minnesota were randomly assigned to D.A.R.E., D.A.R.E. Plus, or a delayed program. There were 6,237 students at baseline, and 84% were surveyed at the one-year follow-up. Used growth curve models to account for missing data.
D.A.R.E. Plus (Perry et al. 2003)	D.A.R.E., plus four sessions of a peer-led program, extra-curricular activities, and neighborhood action teams.	
Hutchinson Smoking Prevention Program (Peterson et al. 2000)	Sixty-five sessions between 4th and 10th grades; comprehensive social influences.	Forty school districts in Washington were randomly assigned to intervention or control condition. The study started with 8,388 3rd grade students who were followed to two years after high school (94% follow-up).
Keeping it R.E.A.L. (Hecht et al. 2003)	Ten sessions in middle school (with booster activities and advertising); resistance and life skills.	Thirty-five school districts were randomly assigned to intervention or control condition (n = 6,035 respondents; used multiple imputation to address attrition and missing values). Final wave of interviews was conducted fourteen months postintervention.
Know Your Body (Walter, Vaughan, and Wynder 1989)	Two hours of instruction each week during the school year from 4th to 9th grade; normative and stress management.	Fifteen schools in the vicinity of New York City were assigned to either an intervention or a nonintervention group (n = 1,105 eligible children, 911 participated at baseline, and 593 were interviewed at six years).
Life Skills Program (Botvin et al. 1995)	Fifteen sessions in 7th grade (boosters in 8th and 9th); cognitive behavioral resistance skills.	Fifty-six schools were randomly assigned to intervention or control; 3,597 12th grade students represented 60.61% of the initial 7th-grade sample.
Life Skills Program (Botvin et al. 2001)	Fifteen sessions in 7th grade (boosters in 8th); cognitive behavioral resistance skills.	Twenty-nine New York City schools were randomized to receive the intervention or be in the control group; 5,222 students (predominantly minority) participated in the study, and 69% provided data at the one-year follow-up.

Alcohol use	Tobacco use	Illegal drug use
"Students receiving the normative education program had significantly lower average levels of reported cigarette and alcohol use, lower rates of growth for reported cigarette and alcohol use, and less deceleration of reported levels of cigarette and alcohol use as compared with the control group (information about consequences of use)."		Analyses based on five waves of data did not discuss illegal drug use. At one-year follow-up, normative education group demonstrated lower rates of recent marijuana use (2.2% vs. 6.2%; $p < 0.001$).
No significant differences in outcomes between students assigned to D.A.R.E. and students assigned to the control condition.		
"Among boys, those in the D.A.R.E. Plus schools were less likely than those in the control schools to show increases in alcohol use behavior and intentions, past year or past month alcohol use, tobacco use behavior and intentions, multidrug use behavior and intentions, and victimization" ($p < 0.05$). There were no significant effects for girls.		
	"No significant difference in prevalence of daily smoking was found between students in the control and experimental districts . . . Moreover, no intervention impact was observed for other smoking outcomes, such as extent of current smoking or cumulative amount smoked, or in *a priori* specified variables, such as family risk for smoking."	
Mean difference in past month use between intervention and control after accounting for baseline level: –0.232 ($p < 0.001$).	Mean difference in past month use between intervention and control after accounting for baseline level: no significant difference.	Mean difference in past marijuana month use between intervention and control after accounting for baseline level: –0.175 ($p < 0.001$).
	13.1% of control group had initiated use in 9th grade compared to 3.1% of those assigned to the intervention ($P < 0.005$).	
The intervention did not influence frequency of use at the six-year follow-up, but it did reduce the probability of being drunk (40% vs. 34, $p < 0.05$; vs. 0.33, $p < 0.01$).	The intervention reduced past-week smoking (33% vs. 27, $p < 0.05$; vs. 0.26 $p < 0.01$) and past-month smoking (27% vs. 23, $p < 0.05$; vs. 0.21 $p < 0.05$).	Had no effect on marijuana use at the six-year follow-up.
The authors measure substance use on a variety of scales, with 1 = "Never" or "I don't drink," and a maximum ranging from 6–11, depending on the measure. The mean scores for treatment and control groups both hovered between 1 and 2. The scores for the control group were marginally higher and statistically significant for the following measures: smoking frequency*, smoking quantity**, drinking frequency**, drunkenness frequency*, drinking quantity**, inhalant frequency*; $p \leq 0.05$*; $p \leq 0.01$**. There was no statistically significant difference for marijuana frequency or getting "high" frequency.		

(*continued*)

Table 7.1 (continued)

Study	Program description	Evaluation design and sample
Project ALERT (Ellickson, Bell, and McGuigan 1993)	Eight sessions in 7th grade (boosters in 8th grade); social influence model—resistance skills training curriculum.	Thirty schools in California and Oregon were randomly assigned to three conditions: ALERT taught by health educators, ALERT taught by health educators with help from students, and a control; ~4,000 students were assessed in 7th grade, and six times thereafter through grade 12. The analysis sample at grade 12 constitutes 57% of baseline sample.
Project ALERT Plus (Ellickson et al. 2003)	Eleven sessions in 7th grade (boosters in 8th grade); social influence model—resistance skills training curriculum.	Fifty-five middle schools in North Dakota were randomly assigned to ALERT, ALERT Plus (with high school booster sessions), or control. Of the 5,412 students enrolled in these schools, 4,689 completed baseline survey and 4,276 completed follow-up at eighteen months after baseline.
Project ALERT (St. Pierre et al. 2005)	Eleven sessions in 7th grade (three boosters in 8th grade); social influence model—resistance skills training curriculum.	Eight schools in Pennsylvania randomly assigned two 7th-grade classrooms to each of three conditions: (1) adult-led Project ALERT; (2) adult-led, teen-assisted Project ALERT; and (3) control. Participants were recruited before 7th grade and followed-up through 9th grade. There were 1,649 participants, and 88% completed the questionnaire in at least four out of five waves.
Project SHOUT (Elder et al. 1993; Eckhardt, Woodruff, and Elder 1997)	Eighteen sessions in 7th and 8th grade (boosters in 9th and 11th grade); comprehensive social influence.	Twenty-two schools in San Diego were randomly assigned to intervention or control condition. There were 3,655 participants, and 2,688 (73%) were available to be surveyed at the end of 9th grade.
Project toward No Tobacco Use (Dent et al., 1995)	Ten sessions in 7th grade (one booster in 8th grade); comprehensive social influence.	Forty-eight schools were randomly assigned to the intervention or control group (N = 6,716); 52% of the sample was interviewed at the twenty-four-month follow-up.
Project towards No Drug Use (Sun et al. 2006)	Twelve sessions in one year of high school; health motivation, social skills, and decision making curriculum.	Twenty-one schools were randomly assigned to control, classroom only, or classroom + (SAC). Of 1,578 baseline subjects, four–five-year follow-up data were available for 46%.
Start Taking Alcohol Risks Seriously (Werch et al. 2003)	One session in 6th grade and one in 7th grade. Materials sent to home. Second session is with nurse.	650 sixth-grade students were randomly assigned to the intervention or a minimal intervention control (a booklet to read at school).

Source: Heavily based on table 4 in Skara and Sussman (2003).

Alcohol use	Tobacco use	Illegal drug use
At the six-year follow-up: "Once the lessons stopped, the program's effects on drug use stopped. Effects on cognitive risk factors persisted for a long time (many through grade 10), but were not sufficient to produce corresponding reductions in use" (856).		
ALERT Plus did not influence alcohol initiation or current use, but it did lead to lower alcohol misuse scores ($p < 0.05$), and students were less likely to engage in drinking that resulted in negative consequences ($p < 0.04$).	ALERT Plus reduced cigarette initiation by 19% ($p < 0.01$) and past-month smoking by 23% ($p < 0.01$).	ALERT Plus reduced marijuana initiation by 24% ($p < 0.01$).
Analyses failed to yield any positive effects for substance use or mediators for use in the adult or teen-assisted delivery of the curriculum.		
	"At the end of the third year, the prevalence of tobacco use within the past month was 14.2% among the intervention students and 22.2% among the controls . . ." ($p < 0.001$). Weekly cigarette use increased 9% for controls and 5% for those assigned to intervention ($p < 0.05$). Trial cigarette use increased 23% for controls and 16% for intervention ($p < 0.05$).	
"[S]ignificant reductions were not found for 30-day use of cigarettes, alcohol, or marijuana use" (191).		Adjusted mean levels of 30-day hard drug use at 4/5 year follow-up: control (1.51%); class (0.66%), SAC (0.3%), $p = 0.02$. The authors note that this effect was evident at one-year follow-up, but not at the two–three-year follow-up.
At the one-year follow-up: "While mean alcohol consumption on all four measures of use was lower for neighborhood students receiving the intervention as compared to the control condition, these differences were not significant."		

it is unclear whether these effects persist after the program is over and how attrition influences the results. The six-year Life Skills evaluation included 61 percent of the initial seventh-grade sample and also found that it decreased smoking. Finally, AAPT noted a significant effect on cigarette use for those receiving the normative education program.

There is a long-term evaluation that did not utilize a randomized controlled design that should be mentioned because it directly assessed the impact of the intervention on substance use and schooling outcomes. The Seattle Social Development Project focused on teacher training, skill development for students, and parent training. There were three conditions: "Full"—at least one semester of intervention in grades one to four and at least one semester of intervention in grades five to six; "Late"—at least one semester of intervention in grades five to six; and the "Control" received no intervention. Hawkins et al. (2005) were able to follow-up with 94 percent of the participants ($n = 605$) when they were twenty-one years old (nine years after the intervention). While they did not find noticeable effects on substance use,[7] they did find statistically significant effects in terms of functioning at school or work at age twenty-one among the treatment group. The authors attempt to reconcile the contradictory findings by stating that "it is also possible that at twenty-one years of age, the use of various substances is relatively normative, even among those progressing positively in the domains of school and work" (Hawkins et al. 2005; 30). If this argument is correct, it may be the case that the programs highlighted in table 7.1 do influence human capital development even if they have no noticeable long-term effect on substance use.

It is also important to acknowledge that even if these school-based prevention programs do not have a long-term effect on consumption, the fact that they delay initiation of particular substances for some students may influence subsequent educational attainment and labor market outcomes. Using two-to-three year follow-up data from a variety of middle school-based prevention evaluations (both randomized and nonrandomized), Caulkins et al. (2002) calculated the initiation effects of a composite, hypothetical "best practice" prevention program on alcohol, tobacco, and marijuana use.[8] They estimate that a 1 percent reduction in substance use observed twelve months after the end of a good prevention program could generate anywhere from a 14 to 51 percent reduction in lifetime quantity consumed of that substance, depending on the substance targeted by the prevention program. Even if the true effect on initiation is indeed the lower bound, this is not negligible.

7. Specifically, they note that the full-intervention group participants "were also less likely to have used a substance in the recent past (alcohol or tobacco in the past month or any other illicit drug in the past year), but this finding did not achieve statistical significance ($p = 0.09$). Subsequent analyses examining different substances separately found no significant effects of the full- or the late-intervention condition, compared with controls, for past month alcohol or tobacco use or for past year marijuana or other illicit drug use" (29).

8. The authors considered four approaches for generating these ranges and fully acknowledge the limitations.

A further point to consider about these programs is that school-based drug prevention programs are relatively inexpensive to implement on a per child basis. A study by Aos et al. (2004) reports the cost of a variety of school-based substance abuse prevention programs and compares these program costs to that of a variety of general prevention programs, community initiatives, and mentoring programs. They show that the per child cost of many of the school-based programs just mentioned range from a low of ~$5 dollars per child (for Project ALERT and TNT, respectively) to a high of only $112 (for DARE).[9] In all cases, however, the costs exclude the cost of teachers' time spent training and preparing lessons, as well as the opportunity cost of the time that could have been used teaching alternative subjects. Most of the school-based programs are on the lower end of the range, with Life Skills and Start Taking Alcohol Risk Seriously (STARS) costing $33 and $20, respectively. The one exception was the Seattle Social Development project, for which they report a per child cost of $5,172, but these program costs include teacher training and parent training on top of the interventions planned in early and later grades.

The fact that the average cost per child of implementing these programs is so low means that the programs do not have to have very large outcome effects in order to have a benefit-cost ratio greater than one. Caulkins et al. (2002) point out in their assessment of an ideal school-based drug prevention program that 95 percent of the time the benefits are more than twice the cost of actually implementing the program on a per child basis, even when programs are assumed to have small effects. Of course, the fact that something creates cost savings does not mean that it is desirable to implement, as it depends on several factors, including the relative cost-effectiveness of other approaches that could be used to achieve the same end.

7.5.2 Family-Based Programs

As previously noted, many prevention programs include a school and home component. The latter can be as passive as sending anti-drug materials home to something as active as including parent training on how to talk to adolescents about substance use. This section focuses on those interventions that primarily focus on the family, and like the earlier section, we focus on the latest wave of outcome data.

The program that receives the most attention in the review literature is the Iowa Strengthening Families Program (ISFP). The program lasts seven weeks, with parents and students meeting for two hours per week with trained program staff. In the first hour the parents and students are separated for their sessions, and in the second hour they are all brought together. Sessions primarily focus on parenting skills, peer resistance, and communication skills. Schools were randomly assigned to ISFP or a minimal contact control condition and "nonlinear growth curve analyses were

9. Aos et al. (2004) report all figures in 2003 dollars and we converted them to 2007 dollars.

conducted with school-level outcome variables aggregated over the available respondents in each school . . ." (Spoth et al. 2004). Analyses based on the six-year follow-up (for students with a data available at all waves, case-wise deletion was used for those missing any information) found that time to alcohol use without parental permission, drunkenness, and cigarette use was significantly longer for those assigned to ISFP ($p < 0.05$).

As part of the evaluation, Spoth et al. (2004) also randomly assigned some schools to a related program called Preparing for the Drug Free Years (PDFY), which primarily focuses on the parents separately and includes fewer sessions. Compared to the same control group, there was no difference in time to initiation for any of the substances for those assigned to PDFY, but there was a statistically significant difference in growth rates for tobacco use ($p < 0.05$).

The Focus on the Family program is intended to prevent substance use for children of heavy drug users in treatment (Catalano et al. 1999). The program included thirty-three hours of parental skills training and nine months of home-based case management. A total of 140 adult methadone patients (and their 178 children, ranging from three to fourteen years) were recruited and either assigned to the intervention or a no-intervention control. The program did not have much of an effect on the children at the one-year follow-up interview, but it did influence parental drug use.

Family-based programs are generally more expensive per youth than school-based prevention programs because they involve more people and management. For example, Aos et al. (2004) report that the average cost per youth of the Iowa Strengthening Families Program was $959, which is substantially higher then the school-based prevention programs mentioned before, but lower in cost than the Seattle Social Development Project, which included a school and family component.

7.5.3 Community-Based Programs

In addition to the benefits discussed in chapter 5, participation in the Big Brothers Big Sisters (BBBS) mentoring program has the effect of reducing and/or delaying initiation of substance use. Grossman and Tierny (1998) report that the adolescents assigned to the treatment condition were 46 percent less likely to initiate drug use and 27 percent less likely to initiate alcohol use during the eighteen-month follow-up period (information was not reported for the intensity of use). There are at least two reasons to believe that this study may underestimate the effects of mentoring. First, the authors present intent-to-treat results and nearly 20 percent of the treatment group was not matched to a mentor. Second, it is not clear whether those in the control group were mentored somewhere else, thus possibly diluting the treatment effect.

An experimental evaluation of a related BBBS mentoring program based in schools did not yield the same results (Herrara et al. 2007). Utilizing a

similar waiting list approach with over 1,000 students in ten sites across the country, the study found only one major difference between the control and treatment groups at the fifteen-month follow-up: those in the treatment group were less likely to skip school and more confident that they would attend and complete college.[10] They attribute the lack of effect to attrition from the mentoring (many students switched schools) and noted that the mentors did not have as much time to cultivate relationships as they did in the previous BBBS community intervention.

Aos et al. (2004) estimate that the average cost of BBBS to taxpayers is $1,392 per youth participant; however, this does not include the opportunity costs associated with being a mentor or a mentee. We refer readers to chapter 5 of this volume for a more detailed discussion of these costs and note that the benefits of the community-based BBBS extend beyond a decrease in the initiation substance use.

Another important community-wide prevention strategy is mass media campaigns. Studies have shown these campaigns to be effective at deterring cigarette smoking, binge drinking, and selected drug use in small communities, particularly when coupled with other prevention strategies (Flynn et al. 1994, 1997; Flay 2000; Pentz 2003). However, a careful multiyear evaluation of the National Youth Anti-Drug Campaign, a national campaign funded by the Office of National Drug Control Policy (ONDCP) to prevent kids from using illegal substances, found that the campaign had absolutely no impact on marijuana use among youth (Hornik, Maklan, Cadell, Barmada, Jacobsohn, and Henderson 2003; Hornik, Maklan, Cadell, Barmada, Jacobsohn, and Prado 2003). This was consistent with results obtained from another group of researchers examining the effects of weekly exposure to media ads from the campaign on marijuana use in a single Midwestern state (Longshore, Ghosh-Dastidar, and Ellickson 2006). What is interesting about this latter study is that the researchers found that youth who also received the ALERT Plus drug prevention program, a universal classroom-based social influences and resistance training program, did report lower past-month marijuana use than adolescents exposed to either alone (Longshore, Ghosh-Dastidar, and Ellickson 2006). Thus, there appeared to be synergistic effects of these two programs.

7.5.4 Drug Testing

There are very few studies of the effectiveness of student drug testing (MacCoun 2007; Levy 2009), and fewer still that employ a randomized design. We are aware of only one study using a randomized design to evaluate the effects of student drug testing in schools (Goldberg et al. 2007).

10. Specifically, they note: "We did not see benefits in any of the out-of-school areas we examined, including drug and alcohol use, misconduct outside of school, relationships with parents and peers, and self-esteem . . ." (iv).

The researchers recruited eleven schools near Portland that wanted to start student athlete drug-testing programs. Five of these schools were randomly assigned to implement testing programs and the other six were assigned to defer implementation until the study was completed (653 student athletes in testing schools and 743 student athletes in the deferred testing schools). Substance use was serially assessed with voluntary, confidential questionnaires. The results of the two-year prospective study were mixed: testing did not influence past-month drug use among student athletes, but "prior year drug use was reduced in two of four follow-up self reports, and a combination of drug and alcohol use was reduced at two assessments as well" (421). Even though this study used a randomized design, we cannot draw strong conclusions from the results for at least three reasons: (a) five of the eleven schools were removed from the study after originally agreeing to participate and being selected; (b) two of the remaining schools altered their testing programs during the study; and (c) those schools that remained in the sample experienced high attrition among the student athletes.[11]

Another drug testing experiment conducted by the California Youth Authority (CYA) also generated inconclusive results. Nearly 2,000 young parolees (twelve to twenty-four years old, mean nineteen years) were randomly assigned to one of five different levels of testing (including no testing), and graduated sanctions were supposed to be applied to those testing positive (Haapanen and Britton 2002). Those assigned to higher levels of testing were more likely to be arrested for a violent crime and less likely to have a "good" parole outcome at twenty-four months postrelease (Haapanen and Britton 2002). The study could not address the causal effect on drug use since self-reported drug use information was not collected, although the authors noted that parolees assigned to lower levels of testing were more likely to test positive.

A follow-up analysis of this experiment focusing on human capital outcomes and accounting for noncompliance found that parolees randomly assigned to testing were more likely to be employed or in school in the month after being released from prison, with the effect being large for Hispanics and nonexistent for blacks (Kilmer 2008). The lack of long-run employment and schooling data for these parolees makes it difficult to reconcile these findings, but it suggests we have more to learn about the heterogeneous and dynamic effects of drug testing in criminal justice settings.

Drug testing costs vary greatly depending on the testing method, the substances being tested, and whether the entity purchasing the test receives a quantity discount. For example, urine tests conducted by criminal justice

11. An important study by Yamaguchi, Johnston, and O'Malley (2003) using a quasi-experimental design to examine whether students who attended schools that drug tested students reported less drug use found no significant relationship. The study did not account for the method for drug testing (random or for "just cause"), and hence the results may be biased toward zero because of grouping these two types of drug testing together.

agencies can cost less than $2 per test (with a quantity discount) while over-the-counter hair tests can exceed $60. Full cost evaluations of testing programs should also include the costs associated with addressing positive tests (e.g., additional probation supervision, admission to a treatment program, incarceration).

7.6 Review of the Literature on Substance Use, Educational Attainment, and Labor Market Outcomes

Considerable attention has been given by social scientists to the impact of substance use on worker productivity and labor market outcomes. Substance use is believed to diminish a worker's productivity and lead to poor labor market outcomes for several reasons.[12] First, it may delay initiation into the workforce, thereby reducing experience and human capital accumulation associated with on-the-job training (Johnson and Herring 1989). Second, it may decrease the probability of being employed which, again, may interfere with human capital accumulation (Gill and Michaels 1992; Register and Williams 1992). Third, it may increase absenteeism, which directly influences the productivity of not only the worker himself, but also those individuals who work with him (French, Zarkin, and Dunlap 1998). Finally, substance abuse may reduce an individual's productivity at the job, which should translate directly into lower wages if wages are indeed a good indicator of the worker's marginal productivity.

Empirical studies that analyze the direct effect of substance use and abuse on earnings have generated very mixed findings. Even after accounting for the endogeneity of substance use, earnings of substance users are found to be higher by some researchers (Kaestner 1991; Gill and Michaels 1992; Register and Williams 1992; French and Zarkin 1995; Zarkin et al. 1998), and lower by others (Mullahy and Sindelar 1993; Kenkel and Ribar 1994; Burgess and Propper 1998). The lack of a robust finding has led many economists to focus on other measures of a worker's productivity, such as the probability of being employed or unemployed (Kandel and Davies 1990; Register and Williams 1992). Here, too, the evidence is mixed. Using the 1984 and 1985 waves of the National Longitudinal Surveys of Youth (NLSY), Kandel and Davies (1990) find that use of marijuana and cocaine in the past year is positively associated with the total number of weeks unemployed. However, Register and Williams (1992) find, using data from the 1984 wave of the NLSY, that use of marijuana on the job in the past year and long-term use of marijuana both have a positive impact on the probability

12. There is also research examining whether income-support programs encourage substance use by providing resources to support a drug habit, and the evidence is mixed (Shaner et al. 1995; Phillips, Christenfeld, and Ryan 1999; Rosen et al. 2006; Chatterji and Meara 2007).

of being employed. General use of marijuana, on the other hand, did lower the probability of being employed.

The lack of a robust finding is driven by a number of factors. First, studies examine the impact of substance use on earnings and labor market outcomes for populations of varying ages. While some studies focus on young adults (Kandel and Yamaguchi 1987), others focus on mature young adults (Kandel and Davies 1990; Register and Williams 1992), while others focus on the full adult population (Zarkin et al. 1998). It is quite possible that the nature of the relationship between substance use and labor market outcomes changes over the life cycle as job market experience and job tenure begin to dominate the effects of other individual determinants of labor market outcomes. Indeed, a few studies have explicitly considered this fact and noted the differential effects of substance use on wages conditional upon age (Mullahy and Sindelar 1993; French and Zarkin 1995), but it is not a factor that is consistently considered in the literature.

A second factor complicating the interpretation of findings from the literature is the inconsistent treatment of indirect mechanisms through which substance abuse could impact earnings; for example, through educational attainment, health, fertility, and occupational choice. Given that these inputs have been established as important determinants of labor market participation and wages (Becker 1964; Mincer 1970; Willis and Rosen 1979), and that there are strong findings in the literature about associations with each of these (Cook and Moore 1993; Mullahy and Sindelar 1994; Kenkel and Wang 1998; Bray et al. 2000; Chatterji 2006), it is important to consider whether analyses looking at the impact of substance abuse on earnings consider the indirect effects as well.

Finally, the literature is inconsistent in terms of its definition of substance use. "Current" use has been defined as daily use (Kandel and Yamaguchi 1987), use in the past month (Cook and Moore 1993; Chatterji 2006), and use in the past year (Kandel and Davies 1990; Register and Williams 1992; Mullahy and Sindelar 1993). A few studies attempt to differentiate the effects of chronic use from casual use (Kenkel and Ribar 1994; Roebuck, French, and Dennis 2004), or proxy chronic use with measures of early initiation (Bray et al. 2000; Ringel, Ellickson, and Collins 2006). Given all the different ways that substance use can be operationalized, with some representing more chronic or persistent use while others represent more casual use, it is not surprising that findings vary across the studies.

It is clear that the relationship between substance use and abuse and labor market outcomes is dynamic and can be potentially influenced by the relationship between early substance use and human capital production. The potential for reverse causality is also real. Just as substance use and abuse can lead to job separations and other poor labor market outcomes, job separations may lead to increased substance use and abuse. Statistical methods used to date to try to separate out these two effects include event history

analysis (e.g., Kandel and Yamaguchi 1987; Ringel, Ellickson, and Collins 2006), fixed-effects modeling (Cook and Moore 1993; Kaestner 1994a, 1994b; Kenkel and Ribar 1994), and instrumental variable (IV) techniques (e.g., Bray et al. 2000). The problem, however, is not purely statistical in nature. At least some of the problem stems from how and when substance use is measured.

In table 7.2, we highlight rigorous studies that attempt to address issues relevant for understanding the link between substance use and employment, earnings, and schooling. The table is far from exhaustive, as there are many more studies that have been done in this area. However, these particular studies represent major steps forward in the literature in attempting to deal with the statistical and measurement problems so as to get cleaner estimates of the causal associations.

The top of table 7.2 focuses on studies examining the relationship between substance use and employment or earnings. For most of these studies, the major issue has been dealing with the simultaneity of current substance use and current labor market outcomes. The main approach for dealing with the problem is to employ IV methods. The difficulty comes in trying to identify appropriate instruments, and the particular choice of instruments (religiosity, family stability, nonearned income, or illegal acts) used in the first few studies could all be viewed as problematic.[13] The Register and Williams (1992) study, however, remains particularly insightful because it was the first (and, as far as we know, only) study that differentiated the effects of on-the-job substance use from off-the-job substance use. Indeed, they find that in the case of marijuana, off-the-job use was positively associated with earnings, while they find on-the-job use and long-term use to be associated with lower wages. They interpret their results as evidence that recreational marijuana use may help reduce stress in a fashion similar to moderate alcohol consumption. However, since current substance use was instrumented, not long-term use, there is still a possibility that simultaneity bias influences some of their findings.

The results from the two Kaestner studies (1994a, 1994b) demonstrate how substance abuse can differentially influence different aspects of labor market outcomes even for the same population being considered. In the first study, for example, Kaestner (1994a) finds a positive effect of cocaine use on earnings for young adult women, but in his second study (Kaestner 1994b) he shows no effect of the same measure of cocaine use on women's hours

13. The robustness of IV methods depends critically on the validity of assumptions regarding the independence (i.e., lack of correlation) between the identifying "instrument" and the primary outcome of interest (labor market outcome or educational attainment, in this instance) except through its affect on substance use. A variable that is believed to be associated with substance use could also be related to labor market outcomes if it picks up on an unobserved character trait that is relevant to both (such as rates of time preference or acceptance of authority). Instruments used in early studies have been called into question in later analyses that included statistical tests of these assumptions.

Table 7.2 **Literature on the effects of substance use and abuse (SA) on employment, earnings, and educational attainment**

Study	Data	Outcome	SA measure	Methods	SA and other intermediate factors controlled for?	Key insight from study	Effects
				Employment and Earnings			
Register & Williams (1992)	Young (ages eighteen–twenty-six), male workers in the 1984 NLSY	Log annual earnings, hours worked, and probability of employment	Continuous measures of the number of times cocaine and marijuana are used in the past thirty days. Also include long-term use, defined as use eight (five) years for marijuana (cocaine).	Two-stage least square. Identifying instruments include religious affiliation and attendance, divorce status, parental education, and urban status. None are tested for validity or overidentification.	Education, marital status, and health are included as additional controls. Education and marital status are negatively associated with substance use in the first stage.	Cocaine use has no impact on wages or employment. Long-term and on-the-job use of marijuana are negatively related to wages, but off-the-job use is positively associated with wages.	An additional day of off-the-job marijuana use in the past month is associated with a 3% to 5% increase in wages. On-the-job use and long-term use associated with a 73% and 17% reduction in wages, respectively.
Kaestner (1994a)	1984 and 1988 waves of the NLSY79 (sample was twenty-three–thirty-two years old in 1988). Models are estimated separately for men and women.	Log annual earnings	Measures of lifetime and past thirty day frequency of use of cocaine and marijuana. Heavy use measures based on lifetime frequency of use.	Both cross-sectional and panel (fixed effects) estimates are obtained using two-stage least square estimation. Identifying variables include frequency of religious attendance and nonearned income.	Education, experience, health status, and marital status are all included.	No statistically significant association was found between marijuana and log wages in the panel data analysis for males or females. For cocaine, the panel data models show a positive effect of lifetime frequency of use on log wages for females only (significant at 10% level). Women who never use cocaine have significantly higher wages in panel analysis.	A one-unit increase in cocaine use increases wages for women between 28% (cross-sectional model) and 75% (fixed-effect model).

Kaestner (1994b)	1984 and 1988 waves of the NLSY79 (sample was twenty-three–thirty-two years old in 1988). Models are estimated separately for men and women.	Usual number of hours worked in the past twelve months.	Measures of lifetime frequency of use of cocaine and marijuana were examined. Heavy use measures based on lifetime frequency of use were also constructed.	Cross-sectional and panel estimates obtained using two-stage least squares. Identifying variables include frequency of religious attendance and number of illegal acts committed prior to 1980.	Education, experience, and health status are all included. Panel data models are estimated separately by marital status.	The parameter estimates of the effect of illicit drug use were imprecisely estimated and had different signs depending on measure of drug use and the sample evaluated.	No effect.
Kenkel and Ribar (1994)	NLSY 1979 cohort. Separate analyses were done for men and women.	Earnings, labor market hours	Four alternative measures of problem drinking, including DSM-III criteria for abuse, DSM-III criteria for dependence, binge drinking in the past thirty days, and number of days they had a drink.	OLS baseline estimates, individual- and sibling-fixed effects models, and IV models. IV models use price of beer and percentage of state population living in dry counties as identifying instruments.	Marital status, schooling, AFQT IQ test, and health problem indicators that could interfere with work.	When individual fixed effects models are estimated, they find that problem alcohol use is associated with a 1.3% increase in earnings for men and a 1.5% increase for women. When IV methods are used, they find large but statistically insignificant effects of problem drinking on income for males, but large positive and statistically significant effects of problem drinking on females' labor supply.	Given the inconsistent results using alternative methods for handling the problem, no general conclusion drawn.

(*continued*)

Table 7.2 (continued)

Study	Data	Outcome	SA measure	Methods	SA and other intermediate factors controlled for?	Key insight from study	Effects
Mullahy and Sindelar (1996)	Alcohol Supplement of the 1988 National Health Interview Survey, individuals between twenty-five–fifty-nine years of age. Models for men and women estimated separately.	Employed, unemployed, or out of the labor force in the past two weeks before the survey.	Alcohol dependence (DSM-III-R criteria), an indicator of total ethanol consumed, and heavy drinking (90th and 95th percentile for gender-specific distribution).	IV methods. Instruments include state excise tax on beer, state ethanol consumption, and state excise tax on cigarettes.	Schooling, health, and marital status are all included as additional controls.	IV results for males do not support the conclusion that heavy drinking reduces employment or unemployment relative to being OLF. Findings for women is that problem drinking increases both probability of being employed and unemployed (hence lowers likelihood of being OLF). Net effect is ambiguous.	Unclear.
Zarkin et al. (1998)	1991 and 1992 cross sections of the NSDUH, focusing exclusively on young males between the ages of eighteen and twenty-four.	Self-reported hours worked at all jobs in the past month.	Alcohol, cigarette, marijuana, cocaine, and other drug use in the past month and in one's lifetime. Past month alcohol and marijuana use controlled for quantity and frequency using categorical variables.	OLS IV estimation. Instruments include self-reported health risk and difficulty obtaining each substance. Only past month use was instrumented out.	Education, marital status, nonlabor income, number of children, and self-reported health status were all included as additional controls.	In general, they find no consistent relationship between drug use and hours worked. However, light marijuana use (one–three joints in the last month) is positively and statistically associated with hours worked in the 1991 data and negatively and statistically associated with hours worked in the 1992 data. Specification tests support the 1992 results, but the inconsistency in results in adjacent cross sections of the same data is revealing.	1992 results show that smoking between one and three marijuana joints in the past month is associated with forty-one fewer hours worked. 1991 results (although viewed less reliable than 1992 results) suggest that light marijuana use is associated with working forty-two hours more in the past month than nonusers.

DeSimone (2002)	Males from the 1984 and 1988 NLSY79	Binary indicator of having worked at all in the past year.	Binary indicators of any past year cocaine or marijuana use, so frequency or chronic use not captured.	IV methods. Instruments include regional cocaine prices and state decriminalization policies.	Labor force experience, marital status, and number of children are omitted, but educational attainment is included.	Results from the IV models are consistently negative and statistically significant across the years in which the relationship is evaluated.	The probability of being employed is reduced by 15–17 percentage points by marijuana and 23–32 percentage points by cocaine.
				Educational Attainment			
Cook and Moore (1993)	Various waves of the NLSY79 data	High school completion, college entry, and college completion	Heavy and binge drinking in high school.	IV and reduced form methods. Instruments include state variation in beer taxes and minimum legal purchase ages for alcohol.	No.	Heavy drinking during senior year of high school reduces likelihood of finishing high school and graduating college.	Frequent drinkers complete 2.3 fewer years of college compared to nonfrequent drinkers.
Dee and Evans (2003)	NELS:88, repeated cross sections of the 1977–1992 Monitoring the Future Surveys and 1990 Public-Use Microdata Sample	Educational attainment.	Use of alcohol in the past thirty days, binge drinking in the past two weeks.	IV methods and reduced form models. Instruments include beer tax and state minimum legal drinking ages.	No.	Reduced form estimates based on PUMS data show that teen exposure to MLDA of 18 had a small but statistically insignificant effect on high school completion, college entrance, and college completion.	No effect.
Bray et al. (2000)	Longitudinal survey of 1,392 adolescents ages sixteen–eighteen in a southeastern school system.	Dropped out of high school between the ages of sixteen and eighteen (evaluated for each age).	Initiation of each of four substances prior to age sixteen, seventeen, and eighteen. Substances are alcohol, cigarettes, marijuana, and other illicit drugs.	Logistic models of the probability of dropping out of school. Although IV models were tested and rejected, instruments used for identification of age of first use were not described.	One of the first economic studies to fully consider polysubstance use.	Marijuana initiation is positively related to dropping out of high school, although the magnitude and significance of this relationship varies with age of dropout and other substances used.	Marijuana users are at 2.3 times greater odds of dropping out of school than nonusers.

(*continued*)

Table 7.2 (continued)

Study	Data	Outcome	SA measure	Methods	SA and other intermediate factors controlled for?	Key insight from study	Effects
Chatterji (2006)	Year 2000 follow up of the NELS:88. Models estimated separately for men and women.	Four measures of educational attainment by age twenty-six: high school completion, GED, some college, finished college.	Alcohol use in the past thirty days in the 10th and 12th grade, as well as an indicator of binge drinking in the past two weeks from both waves.	Bivariate probit techniques. IVs include state beer taxes and percentage of the state population living in a dry county.	Prior cigarette use in 8th grade.	Results from the bivariate probit models relying on her IVs are unreliable because of the poor performance of her IVs. Attempts to bound the causal association using Altonji et al. (2005) method provide no evidence of a causal interpretation.	No effects.
McCaffrey et al. (Forthcoming)	Project ALERT Plus sample of middle school students in South Dakota	High school dropout status (as reported by school administrator as well as self-reports).	Persistent and heavy marijuana use in the past thirty days in both 9th and 10th grade. Quantity-frequency index of alcohol use in the past month and year.	Propensity score weighting adjusts sample for baseline differences in 7th grade as well as differential participation in the Project ALERT curriculum.	Cigarette use at baseline (7th) and in each wave during high school is considered.	Even after adjusting for propensity weights, marijuana users are 2.3 times more likely to drop out. Statistical significance disappears when measures of cigarette use, family bonds, or peer effects are included.	No effects.

Note: NLSY = National Longitudinal Survey on Youth; DSM = Diagnostic and Statistical Manual of Mental Disorders; OLS = ordinary least squares; AFQT = Armed Forces Qualification Test; OLF = Out of the Labor Force; NSDUH = National Survey of Drug Use and Health; NELS = National Educational Longitudinal Study; PUMS = Public Use Microdata Sample; MLDA = Minimum Legal Drinking Age.

worked. Interestingly, Mullahy and Sindelar (1996) also show a positive effect of heavy drinking on women's labor market participation, suggesting that women with a serious substance abuse problem may be more tied to the labor market.

Findings for men are less clear. Neither of the Kaestner studies shows a consistent result for the effects of cocaine on employment or wages, nor does Register and Williams (1992). However, DeSimone (2002), using the same data set as Kaestner, does find a large statistically significant negative association between marijuana and cocaine use and labor market participation for men. DeSimone emphasizes in his work the importance of instruments satisfying exclusion restrictions and overidentification tests, which he argues are not met by Kaestner's instruments.[14] DeSimone uses external measures of availability, including cocaine prices and marijuana state decriminalization status, for his identifying instruments rather than internal measures of religious attendance, prior delinquent behavior, and unearned income, which were used by Kaestner (1994a, 1994b) and could reflect the same unobserved character trait that motivates both drug use and poor labor market outcomes. DeSimone also employs more proximal measures of substance use, capturing frequency of use in the past year rather than in the lifetime. However, DeSimone does not account for a number of intermediate mechanisms through which substance use might impact labor markets, most notably labor market experience and marital status. So, the omission of these intermediate factors may also contribute to the finding of a large effect.

The Mullahy and Sindelar (1996) paper, which was the last in a series of papers they coauthored examining alcohol dependence and labor market outcomes, was one of the first to carefully test the validity and appropriateness of the instruments used for estimation and consider the extent to which the relationship between alcohol dependence and labor market outcomes might differ over the life course. In another paper they show that the relationship between alcohol dependence and earnings clearly differs by age (Mullahy and Sindelar 1993), but in this study they find no statistically significant or consistent evidence that alcohol dependence influences employment or unemployment, at least for men.

Another question is whether alcohol dependence might influence the types of jobs people get, not just whether or not they work. Kenkel and Wang (1998) use data from the 1979 NLSY to compare job attributes of alcoholic and nonalcoholic men. They find that male alcoholics are less likely to be in white-collar occupations, less likely to receive fringe benefits, and tend to work for smaller firms than their nonalcoholic counterparts.

14. In Kaestner's defense, statistical tests empirically evaluating exclusion restrictions and overidentification in IV models became standard outputs of statistical software estimating these models after Kaestner's work got published.

They also find that alcoholic men who work in white-collar occupations earn about as much as nonalcoholics, while alcoholic men working in blue-collar professions earn about 15 percent less on average. If substance users are able to self-select into specific job occupations that enable them to continue their substance use, then studies that examine the relationship between substance use and wages and other measures of productivity may be biased because they are attributing differences to substance use behavior instead of characteristics of the job.

Finally, using data from the 1991 and 1992 National Household Survey on Drug Abuse, Zarkin et al. (1998) examine the effects of current use (i.e., in the past thirty days) and previous substance use on hours worked in the past month. They use an IV approach, identifying instruments within the survey using information on self-reported risks and availability. They estimate the models separately for 1991 and 1992, both years representing an independent nationally representative cross-section of U.S. households. In general they find no consistently significant effect of any of the substances examined. However, their results for light marijuana users (those reporting use of one to three joints in the past thirty days) were particularly interesting. Using the 1991 cross-section, they found that light marijuana use was statistically significantly associated with working *more* hours (forty-two hours more per month than nonusers). However, using the exact same method, same controls, and the same measure of substance use with the 1992 cross-section, they found that light marijuana use was statistically significantly associated with working *fewer* hours (forty-one fewer hours than nonusers). The authors interpret these completely opposite results despite the same methodology and controls as evidence of the necessity to continue investigating the relationship and the need for careful inspection of models that get estimated.

Schooling outcomes, which are shown in the latter part of table 7.2, are of particular interest because of their close proximity to the delivery of adolescent prevention programs and because educational attainment is such an important factor for labor market outcomes. As in the literature just reviewed on earnings and employment, much of the focus of the schooling literature in economics remains focused on the identification of causal effects, but a much more serious debate over the proper variables for identification of causal effects using IV methods has ensued (Cook and Moore 1993; Dee and Evans 2003; Chatterji 2006). For example, Cook and Moore (1993) use cross-state variation in the minimum legal drinking age and beer taxes, two significant predictors of adolescent drinking behaviors, to identify the causal effects of teen drinking on educational attainment. They find that after controlling for sociodemographic factors and family environment, high school seniors who are frequent drinkers complete 2.3 fewer years of college compared to seniors who are not frequent drinkers. Dee and Evans (2003), however, contend that the approach employed by Cook and Moore is flawed

because it relies on cross-state variation from a single year. They contend that a study of variation in state regulations over time is needed to distinguish these effects from other state-level factors that might affect educational attainment, such as state expenditures on education. Using matched cohorts from the Monitoring the Future Survey and 1990 Public-Use Microdata Sample, they use two-sample IV technique and find that teen drinking has no independent effect on educational attainment. A limitation of their analysis, however, is that average population effects could be driving their null finding as opposed to the nonexistence of a true relationship between schooling and educational attainment at the individual level.

The debate regarding the usefulness of instruments and IV approaches was addressed again by Chatterji (2006), who used data from the 2000 National Educational Longitudinal Study (NELS:88) to model educational attainment at age twenty-six conditional upon current alcohol use in tenth and twelfth grade. Chatterji employed a bivariate probit technique to simultaneously model substance use and specific educational outcomes (high school completion, GED, some college or college completion—each separately), and used as additional instruments state beer taxes and the percentage of the state population living in dry counties. She finds through diagnostics of these instruments that they perform poorly as instruments and decides instead to explore plausibility of a causal relationship using Altonji's et al. (2005) bounding technique. She finds no evidence supporting a causal association using this method. Other strategies have emerged to try to deal with the problem of identification of causal associations in this strand of the literature. Bray et al. (2000), for example, use information from a longitudinal survey and assess whether the age of first use of alcohol, cigarettes, marijuana, and other illicit drugs are statistically associated with the probability of dropping out of school. This sort of prospective approach relies heavily on the notion that early use of these substances (prior to age sixteen) is highly correlated with dependent use later. While this notion is well supported in the literature, it may also be the case that adolescents who initiate at a young age have environmental or personality factors that make them less likely to complete high school (e.g., less parental supervision, bad peers, etc.). They attempt to control for some of these factors, and find that marijuana initiation in particular is positively related to dropping out of high school, although the magnitude and significance varied in a nonlinear fashion with age. What is perhaps most surprising about the study is that early initiation of the other substances was not negatively and statistically associated with high school dropout status, which raises serious questions as to whether it is truly the drug use that is being picked up by these measures or something behavioral.

McCaffrey et al. (forthcoming) use a different approach for evaluating the relationship between substance abuse and schooling. Using a very rich set of panel data from the Project ALERT evaluation, they examine whether

persistent and heavy marijuana use over the tenth and twelfth grade is associated with high school completion using propensity score weighting. They also consider separately the effect of drinking through a continuous quantity-frequency measure of use in the past month and year. They are able to obtain baseline information on the adolescents in seventh grade, before the kids participated in the Project ALERT drug prevention curriculum, and account for a variety of observable differences. When they conduct analyses that simply correct for baseline differences between the groups, including participation in the ALERT program, they find that heavy and persistent marijuana use is still positively associated with high school dropout (odds ratio of 2.3). However, when additional time varying measures are added to a propensity score weighted regression, they find that the statistically significant association between marijuana use and schooling disappears, suggesting that much of the observed association between marijuana use and high school completion can be explained by peer influences and family bonds. The study does not speak directly to the issue of causality, but provides interesting evidence of the mechanisms through which marijuana use might be indirectly associated with schooling. Importantly, the authors find no direct effect of participation in the prevention program on dropping out of high school.

Overall, the findings remain fairly mixed in terms of the effects of substance use on schooling as well as on earnings and labor force participation. While methods that attempt to deal with the endogeneity of substance use generally lead to a reduction in the observed association, the studies using these methods also have problems and instruments have subsequently been found to be either weak or invalid. Thus, the literature continues to evolve, in part because the negative associations remain so strong in observational data and studies are so inconsistent in their treatment of mechanisms through which substance use is allowed to affect the outcomes.

7.7 Summary and Next Steps

This chapter reviews the literatures on the effectiveness of substance use prevention and the effect of substance use on education, employment, and earnings. While there is a fair amount of evidence suggesting that prevention programs for adolescents have short-term effects on consumption, there is very little evidence suggesting these effects remain through high school. But as noted by Caulkins et al. (2002), program effects that last just a year post-program completion can still potentially translate into important changes in terms of lifetime substance use. Research also shows that delaying the age in which a substance is initiated can have a large effect on the probability of becoming dependent and the duration in which the substance is used (Douglas 1998; van Ours 2006; Patton et al. 2007). Thus, it is possible

that these programs could translate into improved schooling or employment outcomes, although the evidence is weak and uncertain.

The economic literature on the casual effect of substance use on education attainment and labor market outcomes remains mixed. While we discussed many factors that have contributed to this situation, perhaps the most important one to keep in mind is how substance use and abuse gets defined in these studies. Clearly, the level of consumption, duration of consumption, and timing of consumption all have important implications in terms of whether we should expect to see an impact on employment or earnings. Rarely have such factors been considered in an analysis.

Another important factor relevant for studying the association between substance use and earnings is the extent to which statistical models are correctly specified for identification of mechanisms that are being considered. While the literature suggests that chronic substance abusers are less likely to be employed, it also suggests that chronic substance abusers are less likely to finish school and more likely to engage in crime. Analyses of the effects of substance abuse on later life cycle outcomes needs to carefully consider the indirect mechanisms through which substance use might also influence those outcomes. The potential for endogeneity bias, caused by unaccounted for differences in ability, antisocial disorders, deviance, mental health problems, rates of time preference, or some other unobserved factor, to impact results abound and few studies have adequately dealt with all of this using IV or other methods.

Information is desperately needed to better inform policymakers of the role substance use might play in contributing to adult poverty. If chronic substance use lowers educational attainment and/or earnings, either directly or indirectly, then programs that prevent or delay substance abuse during adolescence may be an effective way of raising future income and deterring some from becoming economically dependent on the social safety net system or on others. Even if chronic substance use is just an indication of some other third factor that is really driving the correlation between substance use and future labor market outcomes, prevention programs may still be an effective way of reducing poverty—not because they stop substance abuse, but because they teach valuable life skills, resistance training, and coping mechanisms that help empower youth to make better life choices. Moreover, because so many of the prevention programs are relatively inexpensive to implement on a per student basis, they could prove to be an extremely cost-effective strategy for reducing future levels of poverty.

We strongly support additional research on the long-term effects of prevention programs and a more serious look at the direct effect of participation in these prevention programs on economic variables, such as educational attainment and early job entry. These programs are relatively inexpensive and some do show promising results in the short run. If we had to design

the next round of experiments to improve our understanding of prevention programs, we would focus on the following:

- Comprehensive, evidence-based school programs that begin with middle school students and provide sessions through high school. While many prevention programs do have booster sessions after seventh or eighth grade, the typical age for initiation into hard drug use does not come until after high school. One could imagine randomly assigning the grade when school-based prevention stops so we could get a better understanding of whether the timing of booster sessions matters.
- Additional long-term follow-up studies (through senior year of high school) of treatment and control groups for the "evidence-based" programs, with a special focus on human capital accumulation. This would allow us to determine whether program participation shows any real effect on school performance and health by the end of high school, not just substance use. Additionally, this would allow us to better understand the extent to which there is an immediate or slow decay of program effects for youth impacted by the prevention program, and whether additional boosters could prolong program effects.
- A large-scale replication of the Big Brothers Big Sisters community-based mentoring experiment, with a special focus on long-term human capital outcomes, to more carefully evaluate the effect of this program on substance use and economic well-being.
- An experimental evaluation of school-based drug testing that is coupled with a curriculum-based prevention program, paying close attention to alcohol consumption, attendance, and the probability of dropping out of school. Special attention should also be given to the consequences associated with testing positive. Indeed, if the expected sanction for testing positive is small, we would expect the intervention to have very small effects, if any.
- Experimental evaluation of various brief interventions targeted to at-risk youth that examine long-run outcomes. While some of the short-run results are impressive (e.g., D'Amico and Edelen-Orlando 2007), it is unclear whether these approaches have a lasting effect on substance use patterns.

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